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M. J. JOHNSON, Esq. President, in the Chair.

Lieut. Tennant, Bengal Engineers, India;
Rev. S. Newth, New College, London; and
Capt. W. Noble, R.A., Woolwich,

were balloted for and duly elected Fellows of the Society.

On the Computation of Double Star Orbits. By Capt. W. S. Jacob, East India Company's Astronomer.

"In bringing this subject to the notice of the Society, it is proper to premise that I have no decidedly new method to propose; but it has occurred to me, having computed many orbits in various ways, that my experience may be of some service to others, who may feel disposed to take up the subject, by suggesting certain facilities in minor matters of detail, by which time may be saved or greater accuracy attained.

"The plan I follow is, in the main, Herschel's first method given in vol. v. of the Society's *Memoirs;* his second, or improved method, in vol. xviii. I have also tried; but, though more elegant and scientific in form, it does not appear to me to insure greater accuracy, while it has the disadvantage of working *in the dark*, instead of allowing scope for the judgment to be exercised at every step, which, considering the loose character of much of the materials to be dealt with, appears to me of great importance. In order that this second method may be advantageously applied, the observations would have to be all of about the same order of goodness, and pretty equally distributed round a considerable part of the periphery of the ellipse,—conditions which are scarcely as yet attainable in a single instance; and it would also be essential that all the computations should be made in duplicate and compared at every step, since a small numerical error would vitiate the whole results, and would be by no means easy of detection.

"I have, therefore, after a few trials, abandoned this method, and returned to the old or graphical mode with very slight alterations. In the preliminary operations up to the first approximation to the apparent orbit, Herschel's rules are exactly followed; occasionally, when the distance-measures have been sufficiently good and numerous, I have projected them, as well as the angles, into a curve, and have brought the two curves into unison by slight alterations and successive trials; but the occasion for this has rarely offered, and it is in general easy to introduce the effect of the distances in the final corrections. Having then laid down the corrected places from the position curve, I cut out in paper an ellipse of about the proper size, and adapt this so as to agree as nearly as possible with the points laid down; it is then easy to see, not only the proper direction of the major axis, but also whether any variation is required in the dimensions of the ellipse: having thus got approximately the apparent orbit, the real elements are obtained by Herschel's rules, with the following exceptions: having found a' and α graphically, I find it lead to greater accuracy to compute b' and β by the following formulæ rather than to get *them* also by the graphical mode:—

$$\tan B = \frac{t^2}{c^2} \tan A$$

$$a' = \frac{t \,.\, \sec A}{\sqrt{1 + \frac{t^2}{c^2} \tan^2 A}}$$

$$\frac{a'}{b'} = \frac{t \,.\, \sec A}{c \,.\, \sec B}$$

where t and c are the transverse and conjugate axes of the apparent ellipse, a' and b' the projected major and minor axes of the true orbit, A the inclination of a' to t, and B that of b' to c.

"To obtain the mean motion and epoch, Herschel proposes two methods: first, to cut out of card and weigh in a nice balance the whole ellipse and the portions included between the extreme dates of observation and the projected major axis; or second, analytical, by the resolution of certain equations: the first appears to have been the plan generally preferred; the second having been probably rejected by common consent as needlessly laborious. I have not followed either, not having, in general, had at hand a balance accurate enough for the purpose, and being also doubtful of the advantage of the statical mode over that of simple mensuration:—whether, even supposing the weights could be obtained with perfect accuracy, it would be possible to cut out the ellipse more exactly, or even so exactly as it could be measured. My plan is to divide the portion of the ellipse included between the observations into triangles and segments, and measure them in the ordinary way: the area of the elliptic segment being approximately $= 0{\cdot}67\, ch$, where c is the chord and h the height or versed sine of the segment; this is exact for the parabola, and

practically so for the ellipse when the segment is small enough not to differ sensibly from that of a parabola,—say, in general, when c exceeds $10h$. Supposing the observations spread over a large portion of the ellipse, so as to include within them (or nearly so) the area of one or more quadrants, it will evidently be sufficient to measure the *difference* of the total area described from that of the included quadrants.

"For drawing ellipses I have found the best mode, both as regards convenience and accuracy, to be that of co-ordinates, depending on the fact that all ellipses with a common axis (major or minor) have the ordinates on the same abscisses proportional to the *other* axis; consequently, since the circle is one of them, if the absciss* $x = a \sin X$, then $y = b \cos X$.

"There are several of these co-ordinates which can be easily retained in the memory, so that no time need be lost in referring to tables; thus,—

If $x = a \times$	·28	then $y = b \times$		·96
	·50	...	...	·866
	·60	...	...	·80
	·707	...	...	·707
	·80	...	...	·60
	·866	...	...	·50
	·96	...	...	·28

"It will be observed that there are only 4 ratios to be remembered, and they furnish 7 points in each quadrant, besides the extremities of the axes, or 32 points in the whole periphery, through which the curve may be readily drawn with a curve-ruler, such as is used by architects for tracing their mouldings.

"For approximately solving the equation $u - e \cdot \sin u = m$, Sir J. Herschel mentions having constructed a machine of wheel-work: this is, doubtless, a very ingenious contrivance, and probably useful for other purposes; but it is not at all essential here, since a solution may be obtained by the common slide-rule. The line of sines on a Bate's 10-in. rule will give the value by simple inspection to about $0^s{\cdot}1$, or even less; whereas the machine was said to give it only to the nearest degree.

"There is nothing further to be noticed till we come to the final corrections. After comparing the computed with the observed places, I formerly used to correct the elements by the method of least squares. This proved unsatisfactory; the labour was great, and the result by no means commensurate, since the changes required in the elements were often so large as to render sensible the effect of variations of the second and higher orders, and it was not easy to introduce these into the equations of condition; and I now adopt a partly graphical mode, by laying down carefully on a large scale the apparent orbit from the computed places. This will not agree exactly with the one first assumed,

* x having its origin at the centre.

because slight and obvious corrections will probably have been introduced into the elements (especially Ω) in the course of computation: the eye will then readily detect the alterations required in the course or dimensions of the curve to make it agree more closely with observation, and it will sometimes be an assistance to cut out in paper an ellipse of about the right size, and see whether by turning it this way or that the proper amount of deviation can be given. In this way, after a few trials, a result will be obtained as good as the materials are capable of giving.

"It will generally be advisable to compute the coefficients of variation for the different elements, to serve as guides in making these final alterations. They are very easily computed as follows:—

$$\frac{\Delta\theta}{\Delta\tau} = \text{annual motion (apparent)}$$

$$\frac{\Delta\theta}{\Delta n} = \text{annual motion} \times \frac{t-\tau}{n}$$

$$\frac{\Delta\theta}{\Delta e} = \frac{\text{annual motion}}{n} \times \sin u \left(1 + \frac{\sin u}{2 \sin v \,.\, \sqrt{1-e^2}}\right)$$

$$\frac{\Delta\theta}{\Delta(\log\cos\gamma)} = \frac{1}{\text{tab. diff. log tan } (\theta - \Omega)}$$

$$\frac{\Delta\theta}{\Delta\lambda} = \frac{\text{tab. diff. log tan } (v+\lambda)}{\text{tab. diff. log tan } (\theta - \Omega)}$$

"The annual motion is easily deduced from the computed change of angle for any given interval, and the mean distance for that interval, remembering that the rate of motion is always in the inverse ratio of the squares of the distances.

"The coefficients thus obtained may be employed, if it be thought fit, in forming equations of condition for solution by the method of least squares, but it will in general be found preferable to use them, in conjunction with the graphical mode above described, to correct the elements by trial and error; for having found graphically the general direction of the alterations required, the coefficients will enable us to judge on which of the elements the changes can most advantageously be thrown, so as to produce the greatest proportional effect on the resulting angles.

"The following elements for the orbit of *γ Coronæ Australis* were obtained in the manner above described, being the result of the second trial:—

τ = 1863^{yr}·08
P = 100·80
n = 3°·5715
ϖ = 256° 12′
Ω = 352 13
λ = 266 25
γ = 53 35 l. cos = 9·7736
e = 0·602
a = 2″·549

Comparison.

Date.	$\theta o.$	$\theta c - \theta o.$		$\varrho o.$	$\varrho c - \varrho o.$
		∠	arc.		
	° ′			″	
1834·47	37 6	+46	·034		
1835·55	36 48	−48	·035		
1836·43	34 30	− 3	·002		
1837·43	32 42	+ 1	·001	2·66	−·14
1847·32	14 6	+ 5	·003	2·30	−·01
1850·46	5 52	+77	·047	2·29	−·20
1851·54	4 28	− 3	·002	2·26	−·20
1852·27	3 27	−59	·034	1·89	+·12
1852·72	0 58	+13	·007	1·91	+·04
1853·25	359 35	+ 3	·002	1·83	+·08
1853·78	358 30	−30	·016	1·82	+·05
1854·26	356 10	+16	·008	1·71	+·12

" The signs of the distance-errors would indicate some further slight correction of the elements, but the early distances are not worthy of much confidence.

" *London, 7th June*, 1855."

M. Drach has received a letter from Dr. Donati, of the Florence Observatory, informing him of his having discovered, on the 3d instant, a new comet in the constellation *Telescopium Herschelii*. With the ring micrometer, and a low magnifying power, and through mist and clouds, he obtained the following positions :—

1855.	Florence M.T.	Comet — Star. In R.A.	In Decl.
	h m s	m s	′ ″
June 3	10 4 10	−2 17·18	+22 0·0
4	9 55 12	+2 37·78	− 9 22·9
5	9 18 36	−4 2·41	−20 12·5

Comet's App. R.A.	App. Decl.	No. of Comparisons.
h m s	° ′ ″	
.....	.....	2 with (*a*)
6 56 56·27	+36 22 5·5	1 with (*b*)
7 10 32·73	+36 15 15·1	1 with (*c*)

Apparent Positions of the Comparison Stars.

	R.A.	Decl.	Catalogue of Stars.
	h m s	° ′ ″	
(*b*)	6 54 18·49	+36 31 28·4	Lalande 13569
(*c*)	7 14 35·14	+36 35 27·6	— 14298

The star (*a*) is of 10.11 magnitude. Clouds prevented its being observed after the evening of the 3d. The comet does not exhibit any trace of either tail or nucleus. It appeared somewhat fainter than the nebula in *Hercules*.

[This comet is the same as that mentioned in the *Monthly Notices* for May as having been discovered by M. Klinkerfues at Göttingen on the 4th of June. Dr. Donati's discovery is, consequently, a day earlier than that of the latter.—EDITOR.]

On the Values of the Constants of Nutation and Aberration, and of the Parallax of γ Draconis, as Deduced from the Observations made with the Twenty-five Foot Zenith Tube, at the Royal Observatory, Greenwich. By the Rev. R. Main.

The observations discussed in this Paper extend from June 1837 to May 1848, during which time they were made with tolerable continuity, the only interruptions being such as were inevitable either on account of periods of bad weather, or from the difficulty of seeing the star in the day-time, or from occasional readjustments or alterations in the instrument.

The author commences with a preliminary recapitulation of the principal results obtained by preceding astronomers for the values of aberration and nutation. He next gives a description of the instrument employed in the observations which form the groundwork of his own investigation, referring for further details to the Introduction of the *Greenwich Observations* for 1837 and subsequent volumes.

The zenith tube was erected by Troughton during the Directorship of Pond for the purpose of observing several stars in addition to *Polaris*, which pass the meridian of Greenwich very near the zenith, but no regular series of observations suitable for any delicate inquiry was made with it till the accession of Mr. Airy. One of the first points taken into consideration by the latter astronomer was the examination and improvement of this instrument, with the view of rendering it efficient for making an extensive series of observations of *γ Draconis*.

It may be mentioned, for the sake of showing the fineness of the micrometer-screw, that the value of a revolution was found from the mean of several determinations made in the winter of 1836 and agreeing very closely with each other to be $6''\cdot714$; a similar determination in the spring of 1846 gave $6''\cdot724$ for the value of a revolution.

After describing the various precautions which were taken to prevent any disturbing influence from vitiating the observations, the author next proceeds to explain the processes pursued in the calculations.

First, the observations were divided into convenient groups, rarely extending over an interval of time greater than fourteen days; and it is assumed that for these intervals the mean of the observed apparent zenith distances corresponds accurately to the mean of the days of observation. This supposition is sufficiently accurate even with reference to the changes of the coefficients of aberration and parallax.

The author thus obtains a series of normal apparent zenith distances which theoretically ought to be very accurate, and each of which may be represented by an expression containing the following terms:—

1. The assumed mean north zenith distance for an assumed epoch.
2. The correction due for precession and proper motion for the interval of time comprised between the epoch and the day corresponding to the mean of each group of observed zenith distances.
3. The correction for aberration.
4. The correction for parallax.
5. The correction for nutation.

The assumed epoch is 1840, January 1, and the assumed north zenith distance for that epoch is $118''{\cdot}00 + v$, the latter quantity representing a correction, the value of which is to be determined from the investigation.

For the precession the assumed value of n is that given by M. Peters in his *Numerus Constans Nutationis* for 1840, namely, $20''{\cdot}057$; and the assumed proper motion is that deduced by the author himself in his paper on the "Proper Motion of the Fixed Stars." (*Mem. R. Ast. Soc.*, vol. xix.) These, when combined, give $-0''{\cdot}66$ for the annual variation in north zenith distance. For convenience of application a table is made of the amount of this correction for every hundred days.

The constant of aberration is assumed to be $20'' + x$, and the constant of parallax is represented by y, x and y being unknown corrections.

The constant for nutation is assumed to be for 1840: $9''{\cdot}223\left(1 + \frac{z}{10}\right)$, z being an unknown quantity, which, in like manner as v, x, y, is to be determined from the investigation.

The mean zenith distance corresponding to a group after receiving the various corrections above-mentioned computed for the mean date of the group, is compared with the observed zenith distance, and an equation of condition is thus formed, involving besides the three unknown quantities, x, y, z, a constant and unknown correction to the mean zenith distance denoted by v. A comparison of the computed with the observed normal apparent zenith distance for each group furnishes one of such equations, and thus a series of equations of condition are obtained, from which the values of the unknown quantities v, x, y, z, are to be determined.

The total number of equations of condition is 161. Treating them by the method of least squares, the author obtains the following results:—

$v = -0''.173$; weight 528·007; prob. error 0·047
$x = +0''.058$; weight 240·192; prob. error 0·071
$y = -0''.378$; weight 266·231; prob. error 0·067
$z = +0''.118$; weight 289·281; prob. error 0·064

Hence, as the assumed mean distance for 1840, January 1, was $118''.00$; the constant of aberration $20'' + x$; and the constant of nutation for 1840, $9''.224\left(1+\frac{z}{10}\right)$, we have finally,—

Mean Zenith Distance North of γ *Draconis* for 1840, January 1	$= 117''.827 \pm 0''.047$
Constant of Aberration	$20''.058 \pm 0''.071$
Constant of Lunar Nutation	$9''.335 \pm 0''.067$
Constant of Parallax	$-0''.378 \pm 0''.067$

The author concludes with the following remarks:—

"The anomalous results here indicated for the aberration and the parallax seem to show that some cause of error of a periodical character has acted injuriously on the observations; but it is very difficult to imagine a source of error of this nature. A want of fixity of the object-glass, or of the grand micrometer, or of the plumb-line microscopes, would have exhibited itself in producing inconstancy in the zero, and sudden irregularities in the observations; but, on the whole, the zero has, through the whole series of the observations, all desirable steadiness, and the observations are as consistent with each other as could be expected. The value of a revolution of the grand micrometer was determined by an exceedingly accurate method, twice in the interval of a few years, and the two results were almost identical. One determination was made in the winter of 1836, and the other in the spring of 1846; and it is probable that the average temperature was pretty nearly the same at each determination; but, even if the result be affected with an unknown temperature correction, the range of arc required for observations of γ *Draconis* is so small, that no significant error is to be apprehended on this account. The only remaining supposition is that of some derangement or deflection of the plumb-line, and the chance of any disturbance of such magnitude and constancy as the case requires having happened, appears to be very small. If the plumb-line should have been pulled out of the vertical by means of the auxiliary apparatus added in 1840 for the prevention of the twist caused by rapid rotation of the instrument, the deviation would be totally in the prime vertical, and the projection on the meridian plane would still be accurately vertical. It may be added also, that any deviation of the plumb-line from the vertical would be eliminated by the double observation in reversed positions.

"Still, it is believed that, notwithstanding the doubt which, to a certain extent, seems to rest on the results of the investigation, on account of the anomalous values deduced for the aberration and the parallax, considerable weight may still be attached to the value deduced for the nutation-constant, since, on the supposition of an unknown cause of error producing an inequality, having either an annual or a shorter period, its effect would be perfectly eliminated in so long a series of observations.

"The labour bestowed upon the investigation in the endeavour to produce the best possible result from the observations by applying rigorously the method of least squares, has been very great; and, as the whole of the calculations have been performed by myself, I regret much that the results have not proved so unexceptionable as to warrant the amount of care and labour expended. Still, a laborious work of this kind cannot be without its value; and it is hoped that additional scrutiny may afterwards throw some light upon the nature of the errors in the results of an investigation based upon observations made in an unexceptionable way, with an instrument faultless in theory, and pursued by processes combining all the modern refinements.

"For the present I leave this Paper in the hands of astronomers, with the hope that some one interested in the subject may be able to offer some elucidation of the difficulty thus presented.

"1855, *May* 30."

Results of Astronomical Observations made at the Observatory of the University, Durham, from October 1849 *to April* 1852, *under the general direction of the Rev. Temple Chevallier, B.D., F.R.A.S., Professor of Mathematics and Astronomy.* By R. C. Carrington, Esq., B.A., F.R.A.S., Observer in the University. 8vo. Durham, 1855.

In an Introduction, embracing sixteen pages, the author gives a description of the instruments and mode of observation pursued.

From October 1849 to the beginning of May 1850, the observations were almost exclusively meridional; thenceforward, with little exception, equatoreal. The objects observed in the meridian during 1849 and the spring of 1850 were, for the most part, stars from the Catalogue of the British Association, of which modern observations were wanted, the moon and moon-culminating stars, and the planets *Mars*, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*. After April 1850, the objects were mainly such of the comparison stars used for the equatoreal as were not too faint for the transit telescope. The objects observed with the equatoreal were the planets *Pallas*, *Juno*, *Hebe*, *Iris*, *Flora*, *Metis*, *Hygeia*, *Parthenope*, *Victoria*, *Egeria*, *Irene*, and *Eunomia*; and the comets called Petersen's third, Bond's, D'Arrest's, Brorsen's fourth, and Encke's.

The equatoreal observations were published provisionally from time to time in the *Monthly Notices* of this Society, and in the *Astronomische Nachrichten*. Their appearance in a definitive form was, however, delayed, in consequence of the transit-circle of the Durham Observatory being too small for observing the greater number of the comparison stars, and the necessity which therefore existed for obtaining those data from some other sources. The author, before his connexion with the university had ceased, offered to provide for this want as far as he was able, and to superintend the future revision and publication of the observations in a final form, as soon as the star-places were supplied. For a part of these he was indebted to the Astronomer Royal and Professor Piazzi Smyth; the others, or at least those which did not require reobservation, were determined by himself at the observatory which he subsequently erected at Redhill. The total number of comparison stars is 221.

The instruments of the observatory consisted of a small transit-circle of $3\frac{1}{4}$ in. aperture, and 4 feet 2 in. focal length; an astronomical clock, by Hardy; an 8-feet equatoreal by Fraunhofer; and a second equatoreal of 7 feet, presented by the late Duke of Northumberland; besides some smaller instruments not in actual use.

The Fraunhofer refractor with which the equatoreal observations were made had an aperture of $6\frac{1}{2}$ in., and a focal length of 8 feet 3 in.

The first section of the work contains a catalogue of the mean places of stars deduced from observations made with the transit-circle. The second section exhibits the apparent positions of planets observed with the same instrument, and their comparison with the corresponding numbers given in the *Nautical Almanac*. The third section is devoted to observations of the moon and moon-culminating stars, also taken with the transit-circle. The fourth section contains the details of occultations of stars by the moon, observed with the Fraunhofer equatoreal, with the equations of condition deduced from them for the correction of the assumed elements computed according to the method of Professor Challis, given in the *Nautical Almanac* for 1854. The total number of occultations treated in this manner is sixteen. In the fifth section the author gives an account of the determination of the longitude of the observatory by the transmission of chronometers between Greenwich and Durham in the year 1851. The chronometers were conveyed twice to and from Greenwich by railway. The value of the longitude deduced from the first journey was $6^m\ 19^s{\cdot}79$ west; the corresponding result of the second journey was $6^m\ 19^s{\cdot}72$ west. The longitude definitively adopted was $6^m\ 19^s{\cdot}75$ west. From observations of the moon and moon-culminating stars, the author's predecessor had found the longitude to be $6^m\ 19^s{\cdot}1$ west.

The remaining sections are devoted to the equatoreal observations of the small planets and comets, and to the observations of the comparison stars used in conjunction with them. It is to this part of the work that the author more especially invites attention,

under an impression that the final results may prove of some service in the correction of the orbits of the various bodies to which the observations relate.

On the Determination of the Difference of Longitude of the Royal Observatory, Greenwich, and the Harvard College Observatory, Cambridge, U.S.

(*Extracts of Letters from Mr. Bond of Harvard College Observatory, Cambridge, U.S., to Mr. Hartnup of the Liverpool Observatory.*)

"I have again to solicit your valuable assistance in the further prosecution of the expedition for determining the difference of longitude between this place and Greenwich. The results thus far obtained by means of chronometers prove so satisfactorily accordant that Dr. Bache, the Superintendent of the United States' Coast Survey, deems it expedient to continue our operations. With his permission I enclose you the results of the chronometer expeditions of 1849, 50, and 51, which have been recomputed by my son, Mr. G. P. Bond; the details are now nearly ready for publication, and will shortly be sent to press.

"The experience of former years has suggested some precautions and minute attentions, not heretofore, I believe, attended to in any similar expedition, which render it desirable to have your co-operation. If you feel disposed, and your other avocations will permit of an affirmative answer, it will afford me great pleasure.

"*March* 14, 1855."

"My son, Richard F. Bond, and Mr. Philip Sidney Coolidge, will go out to Liverpool on the first voyage, and we are very anxious to have everything previously arranged with the authorities of the Liverpool Custom-house, that there may be no delay in landing the chronometers at your observatory immediately on the arrival of the steamer in the Mersey. We have taken state-rooms in the steamer which is to sail from Boston on Wednesday, the 6th of June.

"A spring-governor, and electric clock for records, will accompany the chronometers; and it will be very desirable to have the temperature maintained as equable and as near that of the mean temperature to which they are exposed on the passage as possible. This being, probably, the last and a test expedition, we shall use every precaution to ensure correct data for the reductions.

"*May* 14, 1855."

Report of the Astronomer Royal to the Board of Visitors, read at the Annual Visitation of the Royal Observatory, Greenwich, 1855, June 2.

The following are a few extracts from this Report:—

"The Twentieth of the Reports which I have had the honour to present to the Board of Visitors will, I trust, be received with indulgence, less on account of the importance of the history which it is to convey, as applying to a year in which no changes of great consequence have been made, than for the numerical place which it occupies in the series of Annual Documents. The date to which the statements of the Report apply is 1855, May 15; and the interval embraced in its historical portion is that between 1854, May 26, and 1855, May 15.

"*Astronomical Instruments.*—On the transit-circle, I have only to remark that it is in perfect order, and continues to give the greatest satisfaction to all the observers.

"The transit-circle at the observatory of the Cape of Good Hope (constructed generally on the pattern of that at Greenwich, with some improvements) is mounted, and is, I believe, in full work. It seems to be quite satisfactory to Mr. Maclear.

"The reflex zenith tube is general good order.

"No alteration has been made in the altazimuth.

"The barrel-apparatus, for the register of transits by punctures produced by galvanic communication, has been in constant use without suffering injury, except in the parts exposed to continual friction, which require occasional attention. The method of giving the time-second-signals from the transit-clock, which I I described in my last Report, is found to be perfectly successful. The insulation of the touch-apparatus has sometimes failed in very damp weather; but, when the sky has cleared, the moistened gutta-percha has become dry and the insulation perfect, so speedily that very few transits have been lost.

"The rest of the galvanic apparatus is, in most respects, in the same state as at the last meeting of the Visitors. In the galvanic magnet for dropping the time-signal-ball, it has been found desirable to guard against the risk of permanent magnetism, by causing the apparatus itself to reverse the poles of the battery at every drop of the ball. When arrangements were originally made for exhibiting the London currents upon the transit-clock needle, and for sending currents to and through London by the touch apparatus of the transit-circle, in order to avoid disturbing the ground, I so connected the wires by turn-plates that one of the wires of the barrel-apparatus was used for these purposes; but with the increased facilities which I now possess for laying wires, I intend to make the barrel-apparatus-wires entirely independent of the others,—preserving, however, the power of connecting the touch-apparatus with the London and foreign wires.

"*Astronomical Observations.*—The well-understood system

of meridional observations of stars remains unaltered, each of the stars in the extended standard list being observed, if possible, twenty times in three years. The moon is observed on the meridian at every opportunity, without exception. In the observation of planets a slight relaxation has been made, which the increased number of those bodies rendered necessary. The small planets are not observed at all in the morning watch (from 3^h A.M. to daylight), and the large planets are not observed then except in company with the moon. With this qualification, the sun and planets are observed on the meridian at every opportunity, except on Sundays.

"The whole number of meridional observations from 1854, May 26, to 1855, May 15, is nearly as follows (an observation of two limbs, or a duplicate transit by eye and ear, and by touch-apparatus, being reckoned as two):—In the transit department; transits, 4680; observations of collimators by means of the transit-telescope, 302; observations of transit-wires by reflexion, 302; observations of one collimator by the other, 52: in the meridian-circle department; observations of all kinds, 4565.

"The number of days on which *γ Draconis* has been observed with the reflex zenith tube is 51.

"With the altazimuth, the number of days of complete observations of the moon during 12 lunations is 178, or 14·8 per lunation, while with the transit-circle the number has been 97, or 8·1 per lunation. The sky has evidently been less favourable than usual; the failing has taken place principally in the spring of the present year. Of the altazimuth observations, 0 are on days when the moon passed the meridian between 0^h and 1^h solar time, 2 between 1^h and 2^h, 4 between 2^h and 3^h, 3 between 21^h and 22^h, 2 between 22^h and 23^h, 2 between 23^h and 24^h. There are no observations on the meridian corresponding to these. The whole number of separate observations of the moon and stars with the altazimuth is 878, and the whole number of observations of its collimator is 604. The observations have been conducted exactly as in the last years.

"The following remarks on the result of the reductions may not be without interest:—

"During the whole time of which I have spoken, the galvanic-contact method has been employed for transits, with the exception of a few days, when the galvanic apparatus was out of order. From the clock-errors, I have deduced the personal equations of the observers in our usual way; not by making special experiments, in which I have very little confidence, but by taking the transits as we find them, and discussing them on the supposition that the clock-rate has been very steady; a supposition in the adoption of which we are amply justified by the comparison of the clock-errors. The result is, that the magnitude of the personal equations in the galvanic-touch method is not above half of that in the eye-and-ear method.

"In former communications to the Visitors, I have alluded to

the constancy of relation between the azimuths of the transit-circle and of the collimators, while all seem to change with respect to the heavens. Other instances of the same thing have occurred from time to time, and have left upon the minds of the computers the impression that there is a real change in the position of the ground. I need not remark that such a conclusion can be received as valid only after very careful and complete discussion.

"*Chronometers, Communication of Time, and Operations for Longitude.*—The number of chronometers now on hand is about sixty; twenty of these being on trial for purchase. They are compared, some every day and some every week, and occasionally in extreme temperatures; the repairs of those which belong to the Government are managed; and weekly reports of rates, and monthly reports of repairs, are made to the Admiralty.

"The system of galvanic normal clock and sympathetic clocks is in the state described in the last year's Report; with this difference only, that the wires to the clock at the Hospital Schools, instead of being suspended across the Park, are carried underground. The clock at the London Bridge Station is made to distribute the galvanic hourly signals to the Electric Telegraph Company's wires and to the different branches of the South-Eastern Railway wires.

"The time-signal ball at Deal was brought into regular use at the beginning of the present year. In a short time, however, its action was interrupted, partly by derangement of the apparatus, and partly by the severity of the weather, which froze the sulphuric acid to the state of jelly. I sent an assistant and workman to put it in order, and since that time it has generally acted very well. Since March 2 there have been three failures; one of these arose from the ball hanging on the clips, which were not properly oiled, and one from the turning off of the current on the Railway line; the cause of the third has not been traced out with certainty. The success or failure of the drop is known immediately at Greenwich; as the Deal ball, at the termination of its fall, so alters the connexions of wires that a signal is sent to the Observatory. A register is kept in a prescribed form in the Ball-Tower at Deal, and copies of this are sent to the Observatory, leaf by leaf, as soon as they are filled. These cautions I consider to be absolutely necessary for maintaining the regular action of the mechanism under rather difficult circumstances. The whole system is so successful that I have no hesitation in recommending its extension to the Government.

"Application has been made to me from one of the important offices of Government, for the galvanic regulation of their clocks. On considering the risks to which various galvanic communications are liable, and the financial necessity for occupying wires as little as possible, I perceived that it was necessary to devise constructions which should satisfy the following conditions. First, that a current sent once a-day should suffice for adjusting the clock, even if it had gone ten or more seconds wrong. Secondly,

that an occasional failure of the current should not stop the clock. I have arranged constructions which possess these characters, and the artist (Mr. C. Shepherd) is now engaged in preparing estimates of the expense. I think it likely that this may prove to be the beginning of a very extensive system of clock regulation.

"I have commenced examination of observations and preparation of skeleton forms for the extension of lunar reductions.

"*General Remarks.*—A fair examination of the statements already made will show how much our real disposable force has been weakened, by accidental circumstances, during the last year. Two Assistants were absent for a month on the Pendulum expedition, and the whole of the extensive calculations which followed were made in the Observatory. One Assistant was employed for some time at Deal, another being at the same time partially occupied in London. The longitude of Paris required the absence of an Assistant for some weeks, and threw a great mass of calculation upon us. For mounting the model of the transit-circle at the Exhibition in Paris, one Assistant has been employed in Paris (with four workmen) for a month. It is under these pressures that I have been obliged to require the assistance of supernumeraries for observations—an assistance which I have been compelled to purchase by the sacrifice of part of their time, otherwise available for computation.

"At the same time, I view the progress of our ordinary work cheerfully. We have effectually maintained the mastery over it; and, if no special cause interrupts us, we shall in a few months have brought everything to the most forward condition in which it is practicable to keep it.

"For the rest, the general policy of the Astronomical Observatory has been the same as during the last twenty years; to leave the equatoreal and scrutinizing departments of Astronomy to other Observatories, but to spare no expense in instruments, no pains in observation, and no labour in reduction, on standard meridional observations generally, and on meridional and extrameridional observations of the moon in particular. And I would fain hope that the *Greenwich Observations* have assumed such a shape, that the Astronomer who may desire to find fundamental determinations of the sun's path, of the places of an extensive catalogue of stars, and of the varying positions of all the moving bodies of the solar system, presented in the utmost extent and fulness, and accuracy, may meet with all that he desires in those volumes."

There were exhibited at the meeting of the Society two beautiful engravings,—one representing two views of *Mars* and the other a view of *Saturn*,—executed from drawings by Captain Jacob, founded upon his own observations of those planets at Madras with the Lerebours equatoreal. One of the views of *Mars* represents the aspect of the planet on the 18th of March,

1854, at $9^h\ 30^m$ Madras Mean Time; the other represents its appearance on the 23d of March, 1854, at $6^h\ 54^m$. The drawing of *Saturn* refers to the appearance of the planet on the 15th of November, 1852. The transparency of the obscure ring which was first remarked by Captain Jacob and Mr. Lassell, independently of each other, is very clearly exhibited in this drawing. The drawings of both planets have been engraved at the expense of the Court of Directors of the East India Company, who have liberally distributed copies of them to the Fellows of the Society and other persons interested in the subject of such delineations. A few copies still remain for distribution, which may be obtained by applying at the Apartments of the Society.

Suggestions respecting the Origin of the Rotatory Movements of the Celestial Bodies and the Spiral Forms of the Nebulæ as seen in Lord Rosse's Telescopes. By James Nasmyth, Esq.

"What first set me thinking on this subject was the endeavour to get at the reason why water in a basin acquires a rotatory motion when a portion of it is allowed to escape through a hole in the bottom. Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate.

"For instance, the minute particles of dew which whiten the glass-blade in early morn are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form!

"Let us pass from the rotation of water in a basin to the consideration of the particles of a nebulous mass just summoned into existence by the fiat of the Creator,—the law of gravitation coexisting.

"The first moment of the existence of such a nebulous mass would be inaugurated by the election of a centre of gravity, and, instantly after, every particle throughout the entire mass of such nebulæ would tend to and converge towards that centre of gravity.

"Now let us consider what would be the result of this. It appears to me that the inevitable consequence of the convergence of the particles towards the centre of gravity of such a nebulous mass would not only result in the formation of a nucleus, but by reason of the physical impossibility that all the converging particles should arrive at the focus of convergence in directions perfectly radial and diametrically opposite to each other, however slight the degree of deviation from the absolute diametrically opposite direction in which the converging particles coalesce at

the focus of attraction, a twisting action would result, and rotation ensue, which, once engendered, be its intensity ever so slight, from that instant forward the nucleus would continue to revolve, and all the particles which its attraction would cause to coalesce with it, would do so in directions tangential to its surface, and not diametrically towards its centre. In due course of time the entire of the remaining nebulous mass would become affected with rotation from the more rapidly moving centre, and would assume what appears to me to be their inherent normal condition, namely, spirality, as the prevailing character of their structure; and as that is *actually* the aspect which may be said to characterise the majority of those marvellous nebulæ, as revealed to us by Lord Rosse's magnificent telescope, I am strongly impressed with the conviction that such reasons as I have assigned have been the cause of their spiral aspect and arrangement.

"And by following up the same train of reasoning, it appears to me that we may catch a glimpse of the primeval cause of the rotation of every body throughout the regions of space, whether they be nebulæ, stars, double stars, or planetary systems.

"The primary cause of rotation which I have endeavoured to describe in the preceding remarks is essentially cosmical, and is the direct and immediate offspring of the action of gravitation on matter in a diffused, nebulous, and, as such, highly mobile condition.

"It will be obvious that in the case of a nebulous mass, whose matter is unequally distributed, that in such a case several subcentres of gravity would be elected, that is to say, each patch of nebulous matter would have its own centre of gravity; but these in their turn subordinate to that of the common centre of gravity of the whole system, about which all such outlaying parts would revolve. Each of the portions above alluded to would either be attracted by the superior mass, and pass in towards it as a *wisp* of nebulous matter, or else establish perfect individual and distinct rotation within itself, and finally revolve about the great common centre of gravity of the whole.

"Bearing this in mind, and referring to some of the figures of the marvellous spiral nebulæ which Lord Rosse's telescope has revealed to us, I shall now bring these suggestions to a conclusion. I have avoided expanding them to the extent I feel the subject to be worthy and capable of; but I trust such as I have offered will be sufficient to convey a pretty clear idea of my views on this sublime subject, which I trust may receive the careful consideration its nature entitles it to. Let any one carefully reflect on the reason why water assumes a rotatory motion when a portion of it is permitted to escape from an aperture in the bottom of the circular vessel containing it; if they will do so in the right spirit, I am fain to think they will arrive at the same conclusion as the contemplation of this familiar phenomenon has brought me to.

"*Bridgewater Foundry, June* 7, 1855."

On Celestial Day Observations. By Thomas Dick, Esq. LL.D.

"During the last nine or ten years more discoveries have been made in the heavens than at any previous period. More than thirty planetary bodies, in addition to those formerly known, have been descried by the unwearied observations of European astronomers. It has frequently occurred to me as not altogether improbable that some new planets might be discovered in the *daytime* in the region of the heavens which lies between the orbit of *Venus* and the sun.

"About the year 1813 I commenced a series of observations on the heavenly bodies in the daytime with a small equatoreal telescope, 20 inches focal length, furnished with powers of 15, 30, 45, and 100 times, with the view of determining the following particulars:—1. What stars and planets may be conveniently seen in the daytime, when the sun is above the horizon; 2. What degrees of magnifying power are requisite for distinguishing them; 3. How near their conjunction with the sun they may be seen; and, 4. Whether the diminution of the aperture of the object-glass of the telescope or the increase of magnifying power conduces most to render a star or a planet visible in daylight. Several hundreds of observations were made in reference to these particulars, and the results were published in vol. xxxvi. of Nicolson's *Philosophical Journal*, for 1813.

"Now, it occurs to me that if a series of observations were made in the daytime on that region of the heavens which lies between the orbit of *Mercury* and the sun, it might be ascertained whether any planetary bodies exist within the orbit of that planet,—a space 37 millions of miles in extent between the orbit of *Mercury* and the central point of the solar system. But such a body could never be detected in the evening after sunset, as its greatest elongation from the sun could not be supposed to be more than 8 or 10 degrees, and consequently it would descend below the horizon in less than half-an-hour after sunset, and before twilight had disappeared. The only chance of detecting such a planet would be when it happened to transit the sun's disk; but as this would only happen at distant intervals, and as it might make the transit in cloudy weather, or when the sun is absent from our hemisphere, there is little prospect of our discovering such a body in this way.

"I am of opinion that it is possible, and not at all improbable, that a planet within the orbit of *Mercury* (if such a body exist) might be detected in the daytime, were powerful telescopes applied to a space of the heavens about 12 or 14 degrees around the sun. Small stars have been seen even at noonday with powerful instruments, and consequently a planet even smaller than *Mercury* might be perceived in the daytime. On the 2d of October, 1843, about 2 o'clock, P.M., I perceived the planet *Venus*, about 2 hours before her superior conjunction with the sun, when only 58′ from the sun's northern limb.

"In making such observations, it is requisite that a round opaque body be placed at a considerable distance from the observer, so as completely to intercept the body of the sun, and about $1\frac{1}{2}°$ of the heavens all around him; and every portion of the surrounding space, extending to at least 12 or 14 degrees in every direction, should then be carefully and frequently examined by good telescopes. The opaque circular board, or other object, in order effectually to intercept the rays of the sun, should be placed at a considerable distance beyond the object-end of the telescope, and and if any contrivance could be made to make it move along with the sun so much the better, so as to prevent any of the solar rays from entering the tube of the telescope. It would likewise be expedient that the object-glass of the telescope should be somewhat contracted. Such observations, if persevered in, would undoubtedly afford a chance of detecting any revolving body that might exist within such a limit. It is not at all improbable that a planet exists within the range of 36 millions of miles from the sun; and from many observations I have made on *Venus* when very near the sun I am persuaded that a body, though only half the size of *Mercury*, could easily be distinguished in daylight with a good telescope.

"*Broughty Ferry, Dundee, May* 31, 1855."

The King of Prussia has presented a Gold Medal to Lieut. Maury, U.S.N., on account of "the distinguished services which he has rendered to science and navigation by his labours in ascertaining the currents and depths of the ocean, and in determining the direction of the winds at different seasons and in different latitudes." This mark of distinction was also accompanied with the presentation of one of the gold medals struck in honour of the publication of Baron Humboldt's *Cosmos*.

The ephemeris of *Amphitrite* for 1855, published in the supplement to the *Nautical Almanac* for 1858, having been found to exhibit a large difference from observation, Mr. Hind has issued a printed circular containing a portion of M. Villarceau's ephemeris of the planet (*Comptes Rendus*, tom. xl. No. 5), adapted to the meridian of Greenwich. This reprint contains the apparent places of the planet from July 1 to September 30 of the current year.

Remarks on the Construction of Telescopes with Simple Cylindrical Glasses. By M. Sturm.

(*Communicated by Dr. Lee.*)

"I take the liberty of calling attention to the application of cylindrical lenses to the construction of telescopes, which are,

as nearly as is possible, free from the defects of aberration: for the special purpose of grinding and centering these glasses I have constructed and patented a machine, and the glasses so ground and centered appear more perfect than those at present manufactured by hand. The defectiveness of the cylindrical glasses now in use is, I think, the reason why they have not been hitherto more used for scientific purposes. I have made a telescope, by way of experiment, with a cylindrical object-glass ground on my machine; the effect of which sufficiently shows the practicability of my method. Although I have at present but one cylinder in my grinding machine, it is, I think, demonstrable, that when I am enabled to construct the several cylinders of the different focal lengths and diameters, important advantages may accrue to the science; among which might be mentioned,—

"Firstly. Having the object-glass single, thus obviating the necessity of different kinds of glass, and the difficulty of procuring flint-glass of a large size free from imperfections.

"Secondly. Object-glasses could be produced of a diameter of from 20 to 24 inches.

"Thirdly. The magnifying power and the extent of field will be proportionately increased, and with equal precision and exactness.

"Fourthly. The time required for manufacturing such object-glasses is considerably less; and

"Fifthly. The cost is greatly diminished."

It may be as well to remind those who possess telescopes of sufficient optical power for the purpose, that it would be desirable to examine the planet *Venus* on the occasion of its approaching inferior conjunction, with the view of ascertaining whether it affords any indications of a twilight conformably to what has been asserted by some preceding observers. (See *Monthly Notices*, vol. xiv. p. 169.)

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